JOHN PIERCE

An Interview Conducted by

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Center for the History of Electrical Engineering

August 19-21, 1992

Interview #141

For the

Center for the History of Electrical Engineering

The Institute of Electrical and Electronics Engineers, Inc.
and
Rutgers, The State University of New Jersey

Interview: John Pierce
Interviewer: Andy Goldstein
Place: Dr. Pierce's home in Palo Alto, California
Date: August 19-21, 1992

Goldstein: I'm here with Dr. John Pierce at his home in Palo Alto, California. It's August 19th, 1992. Let's start from the beginning. Can you tell me something about your birth and early childhood?

Pierce: I was born on March 27, 1910. My parents were Harriet Ann Pierce and John Starr Pierce. They met at Ike Stern's wholesale millinery business in Des Moines, Iowa.

My father had one brother and a number of sisters; they grew up in Williamsburg, Iowa.

My mother had three sisters and grew up in Cedar Falls, Iowa. Her sisters went to the normal school there, which was a teachers' college, and they taught in public schools until they married. For some reason, my mother didn't go to the normal school. She went to Des Moines and went to work. I think she was adventurous. I don't know why. Neither of my parents went beyond high school, both my father and my mother.

Goldstein: You're saying neither finished high school?

Pierce: They finished high school.

My mother, at the time of her marriage, was head trimmer at Ike Stern's. My father was a traveling salesman (I was an only child). My fathers job kept him away from home a good bit of the time. I was really my mother's child. My father was a good man. He was a quiet man, not brilliant, not dumb. My mother had a sharper mind than he did.

In my very early years, I remember spending some months at my grandfather's house in Cedar Falls, Iowa. I don't know just what my age was, but I was pretty

young. My grandfather had a large house on a large lot there, with a barn with a horse and a buggy.

I also visited my father's parents in Williamsburg. They lived in a small house with outside plumbing. My paternal grandfather died when I was fairly young, but his wife lived to have great-great-grandchildren.

The homes I remember when I was five or six or so, were in St. Paul, Minnesota. A little bungalow at 2100 Grand Avenue, which my parents purchased, oh, I would judge about 1916. We lived in two apartment houses before that, but they still might have purchased the bungalow in '16 or '17. On my father's not great income, you could afford to own a house in those days.

I went to school in a portable schoolhouse. This was on the outskirts of St. Paul. I believe that by the time I went to school, we were in World War I because this portable schoolhouse, with its outside plumbing, was a response to crowded, wartime conditions. From that I transferred to other public schools and lived in other places in St. Paul.

My father quit his job at Strong & Warner's wholesale millinery and went into a partnership with his brother, Bill, who was a fairly astute businessman. They had millinery stores in--I believe, one was in Chicago finally, and one in St. Paul, and a couple of others, and took on a ready-to-wear store in Mason City, Iowa.

I spent the last year of grade school then, the eighth grade, and the first two years of high school in Mason City. Then we moved back to St. Paul, and I spent one year in high school in St. Paul. Then my father more or less retired, and we moved to California, first to Long Beach where I finished school at Woodrow Wilson High School.

I was always a tinkerer in my early years and a reader. I believed then that there was wisdom in books, something that I have come to doubt. I read Jules Verne

and was excited by science fiction stories. I read H.G. Wells and was excited about the state of the world. [Incidentally, I had tea with H.G. Wells in 1944.] My mind was full of mixed-up ideas about the world that lay beyond my vision, and confused ideas about the world that I was actually in.

Neither of my parents had gone to college, they thought that I should. And I thought that I should. I knew nothing about California colleges. We were living in Long Beach at this time. I discovered the California Institute of Technology, which fitted my high school education better than any of the others did and sounded like a good thing. I really knew nothing about it. They had an entrance examination. I took the entrance examination, and I got in.

Goldstein: You said that your parents thought you should go to college. Now was that due to the attitude that you showed in high school? Or was there an expectation?

Pierce: Well, I was a very bright student in grade school and high school, and it was very clear, I think, to my parents, if not to me, that I wouldn't succeed at the sort of thing my father did. They were bothered when I was young that I seemed to be not very capable in dealing with the world. Moreover, I suppose it had become natural for people to go to college. All three of my mother sisters had gone to normal school and become teachers. It just seemed natural. I had been interested in technical matters. I had won a hobby show prize when I was in Mason City, Iowa. It just seemed to be in the cards. It wasn't the result of long discussions.

Goldstein: Did you assume you'd be a scientist? Did you know that people worked as engineers?

Pierce: I built a man-carrying glider with two other people just after I graduated from high school or during the last year. We wrote our congressman and got material from the National Advisory Committee for Aeronautics, and we built this darn thing. And it flew. And I'd tinkered with radios for a long time. Oh, and one of

the fellow with whom I built the glider, a fellow named Amo Smith, later became an aircraft engineer. His father was a chemical engineer. When I went to Cal Tech, I thought I would be a chemist. You see, this was the only technical person I had met.

Goldstein: So you say you were a tinkerer. Was that uncommon in the towns that you grew up in? Did many of the kids work with their hands, trying to rig up things?

Pierce: Yes. People built radios. I never became a radio amateur, but I built radio receivers, and read a few of Gernsback's Radio News and other radio magazines. I knew a number of people at high school the year I was in St. Paul who were tinkerers, and I knew some in the high school in Mason City, Iowa, that I attended for two years.

Yes, tinkering was a part of youthful activities in those days. There were lots of things around. I often wonder how children are ever exposed to anything that really works these days. In those days there were still blacksmith shops, there were machine shops. People still lived in towns rather than in suburbs, and there were all sorts of technical activities going on within walking distance. You walked right past them.

Goldstein: Yeah, I can remember as a kid seeing on the back of everything: "No user serviceable parts" inside. There was really nothing to experiment with.

Pierce: You learn with your hands, not with your head. How people get any exposure to the fundamentals of engineering and physics in these days, I often wonder.

Goldstein: I've heard Robert Noyce say that the sort of tinkering you're talking about is--mandatory for people who grew up in the Midwest. There's a lot of equipment on hand. You need to keep it repaired. He may be talking about a specific place and a time. It may no longer be that way.

Pierce: Things have a habit of being unrepairable these days. They're replaced. You don't even ding out the fenders anymore if they're bent. You cut them off and

put new ones on the car. It's a world almost hostile to hands.

In Sri Lanka I received the Arthur C. Clarke Award in 1987, I guess it was. And there's an Arthur C. Clarke Center for Modern Technologies. It's a small government laboratory. It had a touch of reality. One when some of the people got involved in the maintenance and the repair of electronic equipment for the military. Equipment shipped out there, and nobody knows how it works. And this was for real. I visited another laboratory, which was absolutely crazy. They were stirring their fingers, but not very deeply, into everything under the sun, and just word shuffling.

But this business of the reality of things you can actually touch and work. I remember in my early days at Bell Laboratories that we had technical assistants. And some of them, if something broke down, it got better in their hands. And with others it just fell to pieces. There's an ability to tinker constructively. And it isn't deeply grounded in mathematics. You have to know a little bit about things, but some people just have the knack of it.

In those days one of my colleagues, in an effort on the part of the company or himself, to improve matters, got a black technical assistant. This fellow was absolutely hopeless because he didn't know how to tinker. He wasn't dumb. He just hadn't gone through this phase of tinkering that you say Noyce attributed to the Midwest.

Goldstein: I know somebody now who's writing on how technology fares under repairmen's hands, the way its character changes. I can't wait to see what he comes up with because I think it's an interesting question.

Pierce: I was just trying to figure out a few days ago, well, for a week, a circuit diagram of something that the sound engineer, Jay Kadis, built it for me. And I think I've unscrambled its function without asking him, but it was a terrible job for me.

Goldstein: Did you enjoy that sort of challenge?

Pierce: Well, it exasperated me. I thought I was losing my marbles [Chuckling] when I couldn't figure it out.

Goldstein: I want to return to your childhood. You said that you did some tinkering. You even built a glider. How was your hobby was supported? You said your family was poor.

Pierce: They weren't poor. They were middle class. My father made a certain amount of money in business with my father's brother. In California, my parents bought and sold houses, while I was going through Cal Tech and afterwards. They weren't poor. They had investments that they lived on, and they paid for my college education, and for a lot of crazy things that I wanted to do.

Goldstein: You said that you were a good scholar. I think with many people who get involved either in the sciences or in areas called the humanities, that's where their aptitude lies. Their choice is obvious. Your skills were across the board, weren't they?

Pierce: They were sort of miscellaneous. I wrote articles for Hugo Gernsback on how to build and fly gliders probably before I even got to Cal Tech. I didn't know what proof was. He sent me proof not quite telling me what he expected. He found out I didn't know anything about writing. Greenback published and promoted books. One of them was How to Build and Fly Gliders. I was to have filled it up. Well, I filled up a fair amount of it, but he had to fill it up with other material.

I must have been about 18 by that time. I was young for my age in other ways. But that I could write a coherent account of something. Not all people of that age can. And indeed, later on I wished that I could be writer, but I thought it would be more practical to be an engineer, and it was.

Goldstein: You have an interest in writing and poetry. Did you consider pursuing that as a

career?

Pierce: Not seriously. I didn't think it would pay.

Goldstein: So you went to Cal Tech.

Pierce: I went to Cal Tech. I was interested in the courses in literature and history. But I thought in some vague way that I would make my living as an engineer, but I didn't know quite how.

Goldstein: I remember that you wrote you thought you'd be an aeronautical engineer, and just a minute ago you said that you first expected to work in chemistry.

Pierce: Yes. Freshman chemistry cured me of that. I have a great admiration for chemists and chemistry, but it isn't my thing. Then, because I'd built a couple of gliders by that time, two or three, I thought, well, I'll be an aeronautical engineer. My memory, you know--Memory simplifies the history of the earlier days. I did do very badly in drafting. I got tired of drawing rivets and beams, and not very well. And it was a good thing that I was steered off from aeronautical engineering because the first thing they did with people when they went to an aircraft place was put them at a drafting table. I didn't know that.

Goldstein: Can you remember what your objection to chemistry was? Was the theory not satisfying?

Pierce: I just didn't understand what I was being told. The experiments were disasters. I dropped things, spilled things.

Goldstein: Were you looking for a more theoretically-based discipline?

Pierce: They had some theory to go with chemistry, mass actions, and so forth, and that didn't sink in. So I don't know.

I've never been a good experimenter. I did a lot of tinkering, and I have performed experiments but never well. When I got as far as hearing about traveling wave tubes from Rudolf Kompfner. I got the tube shop to build me a traveling wave tube. I drew a picture of it, and they built me one. It worked.

And that was about the depth of the experiments. It wasn't taking a lot of data or interpreting it.

Goldstein: You did some of these things that you wrote about. Your work with electron multipliers and deflection tubes. It sounds like you did do a fair amount of experimentation.

Pierce: I had things built, and I found out whether they worked or not. But taking detailed measurements or seeing that things agreed with theory, in any but the most obvious ways, wasn't my thing.

Goldstein: Right. I guess my question is, did you just not have any patience for it, or no real flair for it?

Pierce: It's certainly a lack of patience. Demonstrations I can deal with. Having something built and seeing that it demonstrates something. Works in a large way. But building the best traveling wave tube in the world because everything is designed well and works well, isn't my thing. I've described myself sometimes as a low-grade theoretician. I've always gone into things early. I got some theoretical grasp of what was going on that was useful. With the traveling wave tube, I had a theory of how things were happening that agreed with the general behavior. But making lots of measurements, or making detailed designs, I have never satisfactorily done. I'm not one of the engineers who designs a bridge from one end to the other and could put it up.

Goldstein: It's that you develop the theory by which other people can put it up?

Pierce: That's about the size of it. And then I get out of the theory because when the mathematicians get in, I can't cope with them anymore.

Goldstein: [Chuckling] So you inspire a line of thought that other people can complete.

Pierce: Yes, I think I have. I remember at Bell Laboratories, I tried to get somebody to do a mathematical analysis relating to synchronization of digital networks, and I couldn't get anyone to do anything. So finally I cobbled together an analysis

myself. And then a mathematician picked it up and wrote a paper I really couldn't understand. [Chuckling]

I tried to get other people to do things. I'm lazy. [Chuckling]

Goldstein: Do you think this has helped your career?

Pierce: Well, it was my career.

Goldstein: When I talk about your administrative role, that's right. But we'll get to it later because we're still now at Cal Tech, and you've given chemistry a shot.

Pierce: I'd given chemistry a shot, and I thought of physics. I've got to separate the undergraduate from the graduate work because by the time I did graduate work, primarily because I didn't get a job. I was getting a bachelor's degree, and I didn't get a job. Then getting a master's degree, and my parents patiently paid for my further years of study. But it's hard to sort out things from this distance. There were good teachers and poor teachers. No one could have taught me chemistry, I think. At some point along the line, I didn't think of physics as something I could do from a sort of superficial reading of the requirements.

I guess that must have been when I decided that I ought to do graduate work because I didn't have a job. And I was probably wrong about that, but it's probably a good thing that I studied electrical engineering rather than physics. Electrical engineering was in the physics department at that time, not the engineering department at Cal Tech.

Goldstein: Did the students have to specialize? When did you have to declare a major field of study?

Pierce: Fairly late, I think. I'm not quite sure. Cal Tech tried in those days to give everyone two or three years of general scientific education. The first year was the same for everyone. I don't know what else to say about Cal Tech. But one thing is that how the teachers worked always seemed to be in accord with how esteemed they were. There's certainly a lot of differences between a good

teacher and a poor teacher.

One thing that you should know about Cal Tech, there were a few lecture courses, graduate courses, in broad topics such as electricity and magnetism. Paul Sophus Epstein taught one. Graduate lecture courses in economics and literature. But most of the teaching was done in small groups of between ten and twenty people, I would say, solving problems, putting them on the blackboard, discussing things with the teacher. Or writing papers in the history or English courses, and getting them marked, and discussing the subject in general. There wasn't a lot of sitting in lecture halls and taking notes, which I find particularly unprofitable.

Goldstein: Yes. You write in your memoirs about one specific professor.

Pierce: Epstein. If I had to copy a formula from a blackboard in order to get it, I'd have been lost totally. Yet I've read accounts of European education in which eminent people deliver very detailed mathematical lectures that are taken down. It's as if somebody never learned to read or write--well, to publish anyway.

Goldstein: Did you recall thinking at the time that you were really learning a field completely? Did you feel like you were able to do a job in engineering if could find one?

Pierce: This is two-sided. While I was a graduate student maybe even before that, but I think it was when I was a graduate student, Mackeown, who was an expert witness in lots of patent cases, had me build pieces of equipment for him, or perform rough experiments for him. And that was one way of earning a living, being an expert witness or an expert in some way. Also, there was a fellow who had worked for RCA Fred Kroger. The idea I had for my thesis was really his. It was a sampling oscillograph. I built one with his money.

Goldstein: This was after you completed graduate school.

Pierce: No, while I was a graduate student. And I took it to him someplace in

Massachusetts, I believe it was. Well, I didn't take it to him. I went and saw him, and it was shipped to him. So I had had a little experience.

In the work for Mackeown I traced the circuits in radios to see if they infringed patents. I also built simple equipment, experimental equipment, for him. And I built this sampling oscillograph. So I knew that those were sorts of things that one might do, but unfortunately I had no actual experience in industry. One of the most wonderful ideas in the world is pursued in Europe. It's called practische in engineering education. It's to have part-time or summer or arranged jobs in industry while you're studying.

Goldstein: I think they were doing that at MIT in the early 20th century. I think GE had a cooperative program.

Pierce: Yes, GE had it, I believe. Cal Tech, never got into anything of that sort.

Goldstein: Was it going on around you?

Pierce: I don't think it was really going on.

Goldstein: I see. But by the time you graduated, you were able to design?

Pierce: Sort of, yes.

Goldstein: I remember reading that you took a theoretical physics class. Was that required for engineers?

Pierce: Most of them took it. The physicists took it in the senior year, the electrical engineers in the first graduate year. It was taught by William Vermillion Houston, and he was a wonderful teacher.

I'll say just a little about the teaching of the formal mathematics courses. I had one from Harry Bateman as a graduate student. For me, mathematics wasn't interesting unless it was tied to some physical problem. I did well in the mathematics courses I took. But I needed to have a sense of the relation of mathematics as a mode of analysis of the world about one. And the simpler, the more common the things to analyze, the better.

Goldstein: Now would that work if the applied problem were something that you weren't familiar with? Or was it closely tied? Did they teach you--?

Pierce: Well, I was exposed to some of the mathematics of heat in two ways: by taking a course that was based around the steam tables, and I think that I got some exposure in some theoretical things. Neither got to me. Either I didn't know enough about steam engines to be interested, or I wasn't interested.

Goldstein: So there was this theoretical physics class. Now that's interesting because you said that you didn't necessarily have an interest in math unless it was applied. Now this theoretical physics class probably didn't have too many direct applications.

Pierce: Somehow Houston made it seem real and interesting, and he taught about Fourier analysis and something about matrices which I believe I didn't quite understand. But he was sympathetic and tried to make things clear and meaningful.

Goldstein: Now had quantum started to work its way into the curriculum at this point?

Pierce: Not into that curriculum. Not into that course. I encountered quantum much later in connection with vacuum tubes and solid state and so on. But it was never a dear friend of mine.

Goldstein: I'm interested in the content of your education, you know, what exactly it prepared you for, and how well prepared you felt you were.

Pierce: It prepared me. It could have prepared me better if I'd taken more mathematics. I'd associated engineering more with things, and I wondered what all this mathematics was about. I took the first third, or the first half of a course on mathematical analysis given by Morgan Ward and did very well. That must have been as either an undergraduate or a graduate, I don't know. But then I thought, What am I doing with this? This isn't something I'll encounter as an engineer.

Goldstein: And would you say that the Cal Tech program was oriented toward the practical or toward the theoretical?

Pierce: Looking back on it, the Cal Tech program gave one a good fundamental grounding in a lot of things, and that was the best thing it could do. When I was later at Cal Tech as a faculty member, I found out that some of the students appeared to develop a prodigious mathematical facility without much attachment to anything physical.

Goldstein: Was it just very abstract math and multidimensional space?

Pierce: It wasn't even that abstract. It was just a computational facility. Or getting really analytical problems and doing them quickly and correctly. I tried giving a course from a book I'd written, Almost All About Waves. I wish I'd written a better book. It was intended to describe the physical properties of waves and relate them mathematically through some analysis. I got the impression that the students had never seen a wave. The mathematics were all right, but the idea that you were surrounded by waves in strings and rooms and electromagnetic waves, and they all have something in common, didn't go across.

Goldstein: You were worried that there wasn't physical intuition about.

Pierce: They seemed to have zilch physical intuition. Or zilch physical curiosity.

Goldstein: This was down in Cal Tech.

Pierce: Yes, this was. I'm not sure whether it was graduate or undergraduate course in EE.

Goldstein: When you were doing your graduate work there, was there one particular area in electrical engineering that was particularly appealing--either to you or to, say, the majority of the students?

Pierce: Radio was of interest, and I thought I'd made an invention as a graduate student in radio; it turned out that I hadn't. Terman's book was known at Cal Tech. I think I taught a course using that as a text. I taught the graduate course

concocted about radio, and microwaves weren't there yet. I remember a friend of mine, a fellow graduate student, James Wilson McRea. He discovered the formulation of waves in terms of retarded potentials. Not a new thing, but a thing that wasn't spoken about among engineers. He taught a course in that. Radio and related electronics were the things that interested me. And there was still some power engineering. Royal Sorenson was a power engineer out of which had grown the million-volt laboratory. But that was on the wane.

Goldstein: I'm glad you brought that up because it was a critical transition time. It seems like power engineering, which had been the heart of electrical engineering, was just starting to stagnate.

Pierce: Yes.

Goldstein: Were people interested in power? Your fellow students?

Pierce: A few graduate students at that time, but not many. Power engineering went off in a very peculiar way. I guess it's still important in some universities. How the need for engineers is filled in the educational world, I don't really know. Because some vital fields just aren't pursued at Cal Tech, for instance. And probably not very strongly at Stanford. There must be universities out there who teach the parts of engineering that prestige schools--the schools that like to think of themselves as prestige schools--don't. MIT has made forays into power, but I don't think that it's really training useful engineers.

Goldstein: Was your education balanced between power and electronics? Or did you concentrate in electronics?

Pierce: Well, I had a course from Royal Sorenson, the head of the department centered around power. At the time I thought it was awfully simple-minded. But a few things have stuck with me that I learned from Royal Sorenson rather than somebody else. One is that if you're thinking of a coil connected across 110 volts, the number of turns and the flux are the important concepts, and not

abstract ideas of inductance. And another was little pictures of square waves or wavefronts being reflected by a transmission line, at opens and shorts and so on. When I came to write about waves in musical instruments, having those pictures of his in mind was much more important than the mathematics I'd learned.

Goldstein: We were just now talking about this sort of radio work you were doing.

Pierce: Well, there's one thing that Mackeown got me in on: That was oil well logging by sending something down the hole and measuring the angle. The work for Mackeown was partly electronic, not just radio. It was things that you could do with vacuum tubes and oscilloscopes and other electronic equipment. I have mentioned the work I did for Fred Kroeger, pursued as a thesis topic, on the sampling oscillograph, which was a measuring tool. Someone came around when I was a graduate student and asked me if you couldn't tell electronically whether oranges were crystallized inside because they split open. Could you do that by putting in a coil and measuring the loss. You couldn't. The loss was all in the skin. I didn't find that out. A physics graduate student who was working on it found that out.

There was a fellow who was a graduate student at Cal Tech when I was who was looking for a way of making a fast buck, and he made sort of phoney electronic medical equipment for quacks. [Chuckling]

Goldstein: Things to measure quantities?

Pierce: Yes. And they would have bells and whistles. He did also make some legitimate diathermy equipment. He was sort of teetering on the line.

Goldstein: I'm curious to know whether your education had much material on the properties of different materials? You talk about the interest in practical matters as opposed to purely mathematical matters. So I wonder whether you learned about, say, the rectifying properties of, you know, copper oxide and so on.

Pierce: No.

Pierce: I didn't hear about this solid state physics with the primitive sort of copper oxide rectifiers and so on until I got to Bell Labs.

Goldstein: Was there room to work on that sort of thing at Cal Tech?

Pierce: Not as far as I know. Not electrical engineers. One fellow who was studying for a doctor's degree in physics--he never got it--worked on solid-state physics ideas principally. He was ingenious and did a lot of good things. But it was all solid-state physics. I gather, that he was particularly effective. But this was on physical properties, maybe was it tin? Something like that.

Goldstein: Did the Depression shape your studies or the studies of some of your fellow students? Was there an additional emphasis to be practical, to be marketable?

Pierce: No, I don't think so. Some of the more practical ones were more practical. One of the guys who got a Ph.D. had worked in connection with some power company, and he got a job with the power company afterwards. Just as an operator, I believe. As I have already said, the great effect of the Depression, as I've already said, on my education was that I went on to a Ph.D. because I couldn't get a job.

Goldstein: Do you think other people were in your situation?

Pierce: I wouldn't wonder if Dean Wooldridge was a graduate student in physics when I was. [Chuckling] I know him only a little. He was working on something that had a lot of glass tubing for gaseous diffusion. I made a sharp noise with a pencil clip and he thought a tube had broken. I don't remember what the purpose was anymore. He went to Bell Laboratories roughly the same time that I did.

I wasn't very well oriented. I didn't know much about the real world of science or engineering. The part of the education that I got that was useful was partly an opportunity to continue tinkering electronically. So that things weren't

foreign to me. And partly I had a fairly good mathematical and theoretical grounding.

Goldstein: Were you surprised when you got to Bell Labs? Did you really feel like you were in a different world?

Pierce: I sure was in a different world. How I felt about it? Well, I was glad to have a job. I was glad to be away from home. I'd lived with my parents up to that time. Not that I didn't love my parents, but it was a restricting environment. And here I was asked to do research on vacuum tubes, initially without any particular guidance and knowing not much about vacuum tubes. I just went ahead and did it. Mine not to question why. [Chuckling]

Goldstein: How did you get your job there?

Pierce: I think that Mackeown got it for me. I'm pretty sure of that. I can't cite chapter and verse. I also went and interviewed at RCA. I was in the East in connection with a trip to Europe that my parents gave me after I got my Ph.D.

Goldstein: So you still didn't have your job when you went on your trip?

Pierce: I had a job, and I accepted it. It was the first job that was offered to me, and it sounded like a good job, and I accepted it. I did interview RCA, but I'd really accepted the Bell Laboratories by that time.

Goldstein: Did Bell Labs have-- Was it attractive because of its prestige? Were you interested in the work they were doing?

Pierce: I didn't know much about them. [Chuckling] Just like wandering into hell, wandering into Bell Labs.

Goldstein: Had you heard of them?

Pierce: Not in the sense of having any deep knowledge of them.

Goldstein: I think Bell Labs hadn't been hiring anyone for a few years prior to that.

Pierce: That's right. I think I was in the second year that they hired anyone.

Goldstein: Was there a large crop of people fresh out of college--out of grad school,

rather?

Pierce: Well, I once had a picture of the people who came in the same year I did. I would think there were twenty or thirty in it. I don't know. I didn't think during those years that I would be interviewed about those years. I've never kept a diary. I was about to say I've never paid much attention to the past, but I'm sort of lost in the present and the future.

Goldstein: Your student recollections are very, very strong. When you said there were twenty or thirty people fresh out of college, did they integrate smoothly with the rest of the Bell Labs staff? If there was a gulf in hiring for a few years did the old guard stay together and the young form a separate group?

Pierce: My impression was that everyone was friendly and did answer your questions. It was a little odd. There were in the department that I was in, a number of people sort of left over from the past, who weren't doing anything very new. There was one, Holden Prescott--who had perfected the silver oxide gaseous photocell that was used in the early days in the movie industry. He was sort of at a loose end at that time. He had a lot of homemade equipment and one or two assistants, and I'm not quite sure what he was doing. I got in touch with him because of photomultipliers.

How I got into photomultipliers was that somebody suggested that I visit Philo Farnsworth's laboratory, and I did. He made photomultipliers as part of his image dissector TV pick-up tube which was of very limited use because it didn't have storage. But it was simple and straightforward. I thought I could make a better photomultiplier. It's from Prescott and his people that I found out how you make photosensitive surfaces. And he was glad to have somebody to talk to and to be helpful.

There was some straightforward vacuum tube work there that some of the people were engaged in, just improving vacuum tubes for higher gain in

negative feedback amplifiers for coaxial cable systems. I tried to do something in that direction. But in the early days I was pretty much left on my own. People were helpful to me if I wanted to learn.

Goldstein: You showed up at Bell, and you were asked to do research on tubes. You weren't given a lot of guidance. They just put you in a lab?

Pierce: They told me where I sat at a desk. When I needed the experimental space, I was told what I could have, and I was told how to purchase things. I would think that if you brought in a new Ph.D. you would put him in some environment where somebody will at least help them to find something to do. But I landed in this unusual time when they were experimenting with this strange practice of hiring that was very much out of touch with having any new people.

Goldstein: Do you remember getting a sense of what the objectives were? For your tube research, did you know that you wanted to decrease the input capacitance, increase the frequency, or something?

Pierce: Either that, or I was told somehow. I was at the Bell Laboratories at the same time as a fellow with a master's degree, C.H. Elmendorf--Chuck Elmendorf--Charles Halsey Elmendorf III, and we shared an apartment.

Goldstein: In New Jersey? Or in New York?

Pierce: In Manhattan. For a year or so. And he was in the trial installations group. I got to know him well. He was later in coaxial cable systems. And I suppose it's through him that I found out that broadband, negative feedback amplifiers were important, and tubes for them were important.

Goldstein: And then you would start working on tubes for those systems?

Pierce: Well, then I was told that what was good about a tube was a low-input capacitance, and high transconductance. It wasn't clear to me at that time to delay would be fatal. Anything that retarded the progress of the signal through

the amblitier was fatal.

Electron multiplication was a great thing in those days. I got into electron multipliers partly through Philo Farnsworth, seeing those that he made which were for optical uses. I also thought that if you could put an electron multiplier after the cathode and grid, that you'd get a high transconductance. Someplace along the line, I learned that the delay would be fatal.

But that wasn't the order in which things happened. Well, I fooled around a lot with tubes with virtual cathodes in them, which seemed to have large transconductances. But really I think it was some sort of radio instability that made the current jump when the voltage changed. I never got anything reproducible out of that. But then I realized that if you multiply the current, you wanted to get a lot of transconductance per unit current. And there's a theoretical limit on that for tubes with grids in them. It has to do with thermal velocities.

But I realized in some way or another--I don't know how--that if you deflected an electron beam there was no such limit in terms of current transconductance per milliampere.

Goldstein: Yes. Now you say it's within the tube that you're deflecting the beam around a corner?

Pierce: Well, the deflecting tube was deflecting the beam within the tube, past an edge. You have a very narrow source of electrons, and then you image that on an edge and use deflection plates to deflect it past the edge. This is a very old idea in vacuum tubes, I later found out. But it does have this peculiarity that even with thermal velocities, there's no limit to the transconductance per milliampere.

Goldstein: You said that you were told that the transconductance should be high?

Pierce: Yes.

Goldstein: Can you remember who said that?

Pierce: Oh, that must have come from Chuck Elmendorf.

Goldstein: And so were there a number of people working on this? Or was this sort of your pet?

Pierce: No. There were a number of people working just to make better conventional pentodes. But I wondered if there would be some entirely different way. At first through the electron multiplication, and that led me into the deflection tubes and the limits on them. And the limits I learned in physical terms from Bill Shockley, I think.

Goldstein: Yes, it sounds like what you're talking about is pretty well-rooted in theory.

Pierce: Yes, it is. And what were the limitations? With electron multiplication and the deflection tube, one could get limitless transconductance per milliampere. But someplace along the line, I learned that the delay in the multiplication would be bad.

Goldstein: Why?

Pierce: It's the phase shift in the feedback path that is the trouble.

Goldstein: You needed to control the delay?

Pierce: No, the feedback loop is closed on itself. If you put in delay, then you're going to have feedback only over a narrow band with frequencies. Before the delay becomes 180 going from negative going to positive, you have to cut the signal off.

Goldstein: All right, so you're working on this matter which does sound pretty theoretical because it's not specific to the application of the tube. What you're talking about is true for all tubes.

Pierce: But it's in the negative feedback amplifiers, that you have to have the slow transit time. You have to have no delays due to electron flows or anything else, or as little as possible.

Goldstein: I guess I want to try to understand what the nature of the other tube research at Bell was. Were people working on decreasing the space between the elements?

Pierce: Yes.

Goldstein: What other sorts of things were people working on?

Pierce: Well, just decreasing the space between the elements and making a smaller, better tube for feedback amplifiers, negative feedback amplifiers, which were the big thing in coaxial cable systems--which weren't there yet.

Goldstein: I thought all amplifiers relied on negative feedback.

Pierce: No, they didn't in those days. Harold Black invented negative feedback before I went to Bell Laboratories, and Hendrik Bode. Nyquist gave the criteria for stability, into which the characteristics of the vacuum tubes and the bandwidth come. And Hendrik Bode worked out the details of the theory.

You needed negative feedback amplifiers in long chains of repeaters because otherwise the gain wasn't stable enough and the frequency characteristics weren't stable enough. The higher you went in frequency the more repeaters you put in. Finally you had one repeater every mile across the country. Now they had to be very stable against the changes in the tube characteristics. So you needed as much negative feedback as you could get, and you needed as much bandwidth as you could get.

What was really done, aside from the ways of controlling or mopping up errors in gain and phase, was to make pentodes, conventional vacuum tubes, with a low input-and-output capacitance, particularly low-output capacitance, and a large transconductance for a given current. And that was done to the end of coaxial systems and negative feedback amplifiers by just improving the tubes.

Goldstein: I know that Bell Labs had a pretty active TV research department, and I think I just assumed that they asked you to go talk to Philo Farnsworth.

Pierce: That was probably because of their TV.

Goldstein: Who assigned you to do that?

Pierce: Well, I had a boss, Mendenhall, who was an experimental physicist graduate of Cal Tech, of the pre-Depression or early Depression era. I had friendly talks with him. He had a boss, Ray Wilson, who was a very sensible and fine guy. Later on Mendenhall wasn't my boss anymore, and J.O. McNally, who was a much more definite character, became my boss. But by that time I had learned to work in the laboratory and publish memoranda and other things. I have lists giving dates and titles of technical memoranda (internal), and published papers and books - I'll refer to these as we talk. I wrote about an ultra-high frequency detector in 11-30-36, shortly after I went to Bell Laboratories. Dated 12-4-36: "High mutual conductance vacuum tubes and d.c. multipliers." From almost the very first, I'd seen Philo Farnsworth's multiplier, and I had had the idea, not original with me, of coupling it with a thermionic vacuum tube. Some people actually built such things. I didn't at that time.

Goldstein: Did you work in block diagrams then?

Pierce: Yes. And then about '35, '37, through my work with Bill Shockley, I was working on designing electron multipliers. But I can see that I had high transconductance in mind when I started to work on multipliers.

Goldstein: Was that a concept that you were familiar with from graduate school?

Pierce: Oh, from the general current technical literature. And then I built one on 4/7/37. This first multiplier design relied on the rubber sheet and rolling balls down the rubber sheet. But that didn't deal with magnetic fields. At that time the best multipliers were multipliers using magnetic fields. So I started off on constructing what was a specialized analog computer, coupled to an electrolytic water tank for getting the electric fields for plotting trajectories in electric and magnetic fields. That was never finished.

Goldstein: You were talking about the rubber sheets with the balls rolling down them. Did

you actually construct a rubber sheet? Was that in an analog computer.

Pierce: Between Shockley and me it got constructed.

Goldstein: I see. I wasn't sure if that was just an analogy you kept in mind.

Pierce: No, this was a real thing, and we designed electron multipliers and published, years later, on these electron multipliers. A fellow by name of Reichman also did some contemporary things at RCA. With Winans in May 1941 I published in the Review of Scientific Instruments, an account of this multiplier research

Goldstein: Maybe I don't understand the difference between the photomultipliers and the electron multipliers.

Pierce: Well, the electron multiplier takes in electrons and produces more electrons at the output by some constant ratio. If the first electrons are photoelectrons, it's a photomultiplier. And photomultipliers, until recently, were at the beginnings of all sorts of electronic instruments that measured light. So an electron multiplier can be a photomultiplier, or you can use the same multiplying effect following an electronic voltage controlled source.

That work was published. There was no place for that in the Bell System. The Bell System had ERPI, Electrical Research Projects, Incorporated, that worked for sound pictures, sound picture recording. They would have a place for electron multipliers. ERPI was eventually sold. At this point it was quite separate from the rest of the Bell System and Bell Laboratories work.

Goldstein: I'm curious about the inspiration for this work. You were saying that it could be used in these negative feedback amplifiers, and that's good for long-distance telephony where you have these repeaters every mile. But were you motivated in improving these systems?

Pierce: No, I saw that the negative feedback amplifiers were really hot things in the whole Bell System.

Goldstein: Here's my question. You were interested in improving the transconductance. Is

that an academic challenge? Or did you think with an improved transconductance I can make a better negative feedback amplifier?

Pierce: Well, with improved transconductance, somebody can make a better negative feedback amplifier. The people who are making negative feedback amplifiers would welcome something with improved transconductance.

Goldstein: You see, this is something you're working on. You're working on photomultipliers, and you conceive of a small analog device, to Then that becomes a challenge in its own right, to get that right.

Pierce: That was just wonderful. It worked from the first. You designed electrodes, And you built tubes.Photomultipliers were built because that's the easiest sort of multiplier. The photoelectric surface is similar to the other multiplying surfaces anyway. And if you want to study multipliers, the whole thing's structure is simpler if it's a photomultiplier.

Shockley and I found electrode shapes in which the electrons leaving one electrode would mostly land on the center of the next electrode, and those leaving that electrode would land on the center of the next electrode. And we had the tube shop build a structure like that. And by gosh! The electrons went to the right place. I believe that we put a little fluorescent material in it one time so we could see where the electrons landed. And it was just about where we expected them to land. And everything worked in a practical way very effectively.

After that I did some other things to try to make the transit time less by putting in high-voltage electrodes between pairs of electrodes, and then shaping the high-voltage electrodes and the other electrodes so that the electrons whizzed right past the high-voltage electrodes. I'm not sure whether that was for transit time or if it was for current output. If you put more and more light on the photoelectrode at the start, you didn't get more and more output. The space

charge defocused the electrons towards the end. That's why I put in the high-voltage electrodes between the active electrodes. And that worked very well, too. You might think that the electrons wouldn't go past the high-voltage electrodes, that they would end up on them. But there are Newton's laws of motion--of inertia--you know. All you have to do is get everything shaped right. How did you get things shaped right? Well, by changing the shapes of the electrodes and seeing where the electrons went. You got a feel for it.

Goldstein: Did you follow your two developments through to manufacture?

Pierce: The first thing I did that was manufactured was reflex klystrons during World War II. There we were essentially up to manufacturing, not quite to manufacture. There's a development department that ran the tube laboratory and fabricated things. And they fabricated things in ways that were suited to manufacture by the manufacturing facilities someplace else.

Goldstein: You said now that you put these high-voltage electrodes in as an innovation. Did you then hand that off to somebody else?

Pierce: There was no one to hand it to. No one was interested in manufacturing it. The electron multipliers --the photomultipliers--weren't of any great use in the Bell System.

Goldstein: Why not?

Pierce: Their biggest use at that time was in talking pictures in the reproducing part, getting the soundtrack amplified. It was Electrical Research Projects, Inc. that was in that business. The company was still owned by AT&T, but it its interests weren't primary to Bell Laboratories, or even serious.

Goldstein: What you're talking about now is the photomultiplier. But the electron multipliers, those did work in stabilizing the chains of repeaters, right?

Pierce: Well, the idea was that they would work. A few months after I went to work. I could tell from the title of a memorandum High Mutual Conductance Vacuum

Tubes and D.C. Multipliers, that's what got me into the multipliers, which I'd seen on the visit to Farnsworth, who wasn't interested in high mutual conductance vacuum tubes. But somebody had already told me, Chuck Elmendorf or somebody else, that high mutual conductance in vacuum tubes was what was needed for this wonderful thing, the negative feedback amplifier. And that was my ultimate aim in working on the multipliers. Although in working on the techniques of the multipliers, it was better to start off with photomultipliers for several reasons. One is they were simple, and another is that putting a vacuum tube cathode and a grid in a multiplier tended to poison the multiplier surfaces, something that one could worry about later.

Pierce: I was told to do research on vacuum tubes, but not very particularly how. I was given some guidance by somebody arranging a visit to Philo Farnsworth's laboratory, which had to do mostly with television. I learned about the importance of negative feedback amplifiers in the future, near and far, of the Bell System, and it was all true. I knew that a vacuum tube should have a better figure of merit, a high transconductance and a low input-and-output capacitance. It was what was needed for improving negative feedback amplifiers.

I found out that it wasn't original with me to think of a vacuum tube that incorporated electron multiplication. But it was original with me to see what some of the limitations were. One was limitation with a conventional vacuum tube as the first starter before multiplication. That there was a limitation on the transconductance per unit current. I tried to do something about that. This led me into the study of deflection tubes. Those are a few statements that all fit together in my mind.

Goldstein: I'm interested in your going down to Farnsworth's lab. Wasn't there any industrial rivalry there?

Pierce: If there was, somebody took care of it. Farnsworth was a very ingenious and impractical inventor, who needed an outlet for his ideas. I went to his laboratory, and I was shown the electron multiplier and the television work.

I was shown also a tube that oscillated and produced radio frequency oscillations that consisted of just a cylindrical electron multiplying surface and a cylindrical grid in the middle. That never came to anything.

I was shown something else that oscillated at radio frequencies; it had metal plates at the ends and the central electrode in the magnetic field guiding these electrons back and forth. That was put forward, I believe, as both a radio frequency oscillator and a radio-frequency-driven electron multiplier. Nothing ever came of that.

I experimented with both of these oscillators, and they didn't seem to me to lead anywhere.

Goldstein: Was the idea that Bell would license things that were of interest?

Pierce: I think that Farnsworth hoped that they would. I had nothing to do with the financial arrangements of the Bell Laboratories in connection with other companies in my time there. But I'm sure that Philo Farnsworth was peddling his wares and that this was a technological visit in which I did the visiting. But if it looked like a wonderful thing, and I said it was a wonderful thing, it seems to me that Philo Farnsworth probably thought he might get something out of it.

Goldstein: You saw what he was working on, and then came back and started experimenting with photomultiplication?

Pierce: The chief thing that came out of that for me, these two RF tubes that he had didn't seem to be any good. His photomultiplier seemed to me to be good, but it seemed awkward to me.

Goldstein: Right. Then you started improving the transconductance. And you said that Farnsworth wasn't interested.

Pierce: That wasn't his thing.

Goldstein: Do you think that was shortsighted on his part?

Pierce: [Chuckling] Look here! Here I land myself in the middle of Bell Laboratories, knowing very little, almost nothing, about vacuum tubes. I hear something good about vacuum tubes, and I go to work on trying to make higher transconductance vacuum tubes. And I've heard about electron multipliers at the same time, so that's how I tried to do it. Farnsworth lived in a different world, largely the world of TV. Somewhat the world of radio. People see different things. It's shortsighted if you don't see anything. That's certain. That's almost being blind. But what you see depends on what's around you.

Goldstein: You'd visited Farnsworth, and he wasn't interested in transconductance. And you were saying that it didn't suit his purposes.

Pierce: As he saw them.

Goldstein: Right. Whereas at Bell, on the other hand, you thought there was a need for it.

Pierce: Particularly with these negative feedback amplifiers for coaxial cable systems.

Goldstein: But I'm still not sure I understand why it is that it wasn't applied in the amplifiers.

Pierce: First of all, the conventional tubes got better, so the competition was higher. Secondly, with the route that I had taken in order to get high transconductance per unit current with a deflection tube, the electrons took a long time to get from the beginning to the end. I hadn't really appreciated that as a difficulty.

Goldstein: Was it simply the physical distance the electrons had to travel that caused the difficulty?

Pierce: Yes, the deflection plate had to be rather long. At least that's the approach I had taken. You see, with a parallel cathode and grid, it's very short in this direction between the cathode grid and the plate. The deflection tube is longer. Also, I was forced to give up the electron multiplication part of this thing because of

the long time it took the electrons to get through the electron multiplier. So I couldn't have that part. So I continued for a while on the deflection tubes, which did have a very low input-and-output capacitance. But with time, the conventional tubes got much better, and it was very clear that they were going to be better all the way along.

Goldstein: Did their characteristics improve in the same way that your electron multiplier tubes did? For instance, you were talking about having a higher figure of merit.

Pierce: They had a higher figure of merit. They were able to make the tubes with smaller cathode-to-grid spacing than before. And eventually a guy named Morton.

Goldstein: Jack Morton?

Pierce: Jack Morton. He made microwave triodes with a very, very small cathode-to-grid spacing. It was done by mounting the cathode on little legs to a glazed section of the ceramic, then grinding the whole thing down so it was flat. And then mounting a very fine grid, winding it on a metal washer, and brazing it to it. Then putting a thin spacer a fraction of a mil thick to space it from the ceramic. And that made the Morton triode, which was used in the first microwave systems.

Goldstein: Let me ask you two questions. First, did you anticipate that phase was going to be a problem with the deflection tube?

Pierce: I didn't really realize its importance until too late.

Goldstein: Not until it had been built?

Pierce: Yes.

Goldstein: Second, were there other divisions at Bell Labs that worked at applying tubes in circuits? Did you get to do any circuit engineering, or was it all component development?

Pierce: Until microwave tubes, it was component development. With microwave tubes,

the circuits were sort of tied to the tubes.

Goldstein: You were working on tubes with high transconductance. Did you try to develop circuits to exploit that difference?

Pierce: No.

Goldstein: Jack Morton came up with the Morton triode. In a case like that, was there conceptual innovation? Or did it simply improve techniques?

Pierce: I can tell you something about that. A fellow at the Radiation Laboratory made a microwave triode with an internal input resonator. I don't think it had an internal output resonator. I'm not absolutely sure. That was Victor Neher who came to the Radiation Laboratory from Cal Tech. He was a physicist and a fine person. He got this narrow spacing. First of all, he was very good with his hands. Experimental physicists build things wonderfully. They used to anyway. He designed the resonator to be in tune at 10 centimeters wavelength with a certain spacing. So the spacing was set by the tuning of the resonator. The resonator was movable by some sort of screw mechanism and he spaced it. This tube had an appreciable gain, about 10 dB, at 10 centimeters wavelength.

I thought, Well, isn't there some more practical way? This shows that a triode with a close spacing can be a good microwave amplifier at 10 centimeters. Isn't there some more practical and reproducible way? I had the idea of mounting the cathode in the center of a ceramic, supported by little struts and grinding a surface plane. And then putting a grid in a plane. Actually,with a thain washer between grid and ceramic. At that time Morton was a new acquisition of the Laboratory. He picked up this idea and made it into the Morton triode, which had an honorable history. At that time it seemed very nice not to have high voltages in the microwave or any other telephone equipment. Later on the traveling wave tube came along, and in the long run it was so much better that the Morton triode became essentially obsolete.

Goldstein: I understand that the traveling wave tube's advantage is the breadth of its bandwidth. It can, for example, amplify over multiple frequencies. It's not so much that it's able to amplify in the S-band. It's just that it--

Pierce: The broad band of the traveling wave tube is very important, and you can put some band-limiting circuits outside of it rather than inside of it, which means you can have much nicer circuits. I was attracted to the traveling wave tube because of its potentiality for broad bandwidth, and that's good. But primarily it's just turned out to be a better amplifier than klystrons or other things.

Goldstein: You thought it was desirable to use lower voltages in tubes.

Pierce: Yes. That was an old Bell System tradition. It was easier to get up to 200 or 300 volts because that was necessary for triodes. But people were suspicious of high voltage.

Goldstein: For what reason?

Pierce: Tradition.

Goldstein: [Chuckling] It wasn't a safety thing?

Pierce: Probably it was a safety thing originally.

Goldstein: What was the name of the person at Rad Lab you said? From Cal Tech?

Pierce: Victor Neher, N-E-H-E-R.

Goldstein: Neher's work at the Rad Lab inspired Morton? Correct?

Pierce: It inspired me, as a matter of fact, and I inspired Morton.

Goldstein: Was your work was supervised during this period? How it was evaluated? How much time did you put in, say, on electron multiplication? What progress did you need to report?

Pierce: None.

Goldstein: You were just given a blank check.

Pierce: Yes. I didn't realize it was strange, but I was given, or I essentially took, a blank check. This started with Dick Mendenhall, who wasn't intrusive. Later, I

worked for Jay O. McNally, and he took a closer look at my work.

The first thing that was any good was reflex klystrons. Sometime around 1938, I started to work on microwave devices. This wasn't entirely a military thing. Bell Laboratories was working toward microwave communication before the war came along. And as a matter of fact, the war interrupted the microwave communication program because you could see that the war was coming and that we were involved in it indirectly, and eventually directly. But the tube work was all bent towards microwave radar.

Goldstein: So Bell was interested in microwaves even before the military applications.

Pierce: Oh, yes.

Goldstein: I can understand why the microwaves are important. For something like radar you need high resolution. What was Bell's interest in microwaves before radar? Why did they get into it?

Pierce: It was a different way of getting broadband communication. The broadband communication developed from the coaxial cable. In the L1 system--I forget the name of it. I don't remember what the repeater spacing was there. But in the last L-system--L3 or L4--I don't remember which coaxial system, there was a repeater every mile going long distances. And it was clear by '36 or '38 or whenever that was, that microwave propagation would allow you to go tens of miles between repeaters and that there was a lot of bandwidth available.

Goldstein: Because the attenuation was less?

Pierce: There are so many dB per mile for a given size cable, and the amount of attenuation goes up as the square root of the frequency. In a broadband system most of the cost of the coaxial system is in the cable anyway, rather than the amplifiers. If you want an economical system, it had better be a broadband system. That means amplifying a broader band and that means putting the repeaters closer together. This was becoming impractical. Well, it was a strain

on things.

The microwave system, on the other hand, offered you zero cost per mile, just a cost per terminal. It also meant a hundred rather than several thousand repeaters between the East and West coasts.

Goldstein: So the equipment was economical.

Pierce: Some people say that it's microwave repeaters that were responsible for the dismantling of the Bell System. Previously it was so expensive to provide transmission before that no one could set up and compete. After microwaves, anyone for good or ill could set up a transcontinental system.

Goldstein: But rivals don't emerge until the late sixties.

Pierce: That's right. But nonetheless, when you did get rivals, they had some alternative. It was possible to go into the long-haul business after microwaves.

Goldstein: When did Bell switch over from its cable system to microwave?

Pierce: It didn't switch over. It built microwaves also. And I think that's sometime in the fifties--I'm not sure just when--early fifties that it started to build microwave systems. And it added them and put them into places where they didn't have coaxial cables. What the Bell System really switched was from cables to was optical fibers, many years later. The microwaves sort of competed with the coaxial cable systems, but they had both.

Goldstein: You've been talking about the different tubes in terms of their use in telephone communications. Even if you weren't getting direction from above, at some point was there input?

Pierce: Yes. I told you that I could see the sort of tubes that you would need for the negative feedback amplifiers that were necessary because there were so many repeaters in coaxial cable systems.

Goldstein: You told me specifically how you were alerted to that problem in this case. Was there any formal system for informing the tube development people about

engineering needs?

Pierce: No, you didn't need one.

Goldstein: Because people were just aware?

Pierce: Yes.

Goldstein: Just good communication?

Pierce: There was a general sort of awareness. And there was a way of being aware. After I got into microwave work, organizationally I was far from Harald Friis's radio research laboratory in Holmdel, New Jersey, but I got into the habit of going out there every month or so to see what was going on.

Goldstein: Right. And you're saying that you started working in microwaves around '38?

Pierce: Yes.

Goldstein: Did the things that you learned working on deflection and electron multipliers, did that help the microwave effort? Or was it a completely different problem?

Pierce: Some of it helped the microwave effort through the tubes.

Goldstein: Even though these things didn't work into the negative feedback amplifiers, like you thought, they still influenced the microwave tuning. In what ways? Can you think of anything specific?

Pierce: Some of these things I didn't understand in 1938. In late '38 and early '39, I was clearly working on microwave tubes. I even had something about electron multipliers in '39 as microwave power amplifiers. Nothing came of that except it shows that I was talking with James Wilson McRae. At the end of 1939 I see something about interconnection of waveguides. Oh, yes, it was not until the beginning of '41 that I actually made a klystron amplifier. It was a low-voltage amplifier.

I've told this story, but I'll repeat it. To get more gain, to make it a low-voltage amplifier, I didn't have just resonators with holes in them. I had resonators with a gridded structure. The grids were mounted on thin copper discs which were

sealed to a succession of glass cylinders. The early klystrons had the resonator in the vacuum, but not this low-voltage tube: here a succession of thin copper disks with grids were sealed between short glass cylinders. Then outside you'd put resonators. This particular tube had a low-voltage grid between the input discs and the output discs. That increased the bunching action. Then there would be an output. Then again the glass envelope. And then at the far end there would be the rest of the glass envelope with a collector electrode. There was a structure to form an electron beam. So the electrons would go through the input gap and get a microwave velocity modulation impressed on them. Then they'd drift down to the low voltage--about 5 volts--electrode. Well maybe if was 5 to 10 volts. And then they'd be re-accelerated and go across the output resonator. And you got quite a lot of amplification there between 10 and 20 dB at S-band.

Now I had a fortunate accident with this tube. I made this low-voltage grid negative accidentally, so that the electrons were turned around and went through the input resonator again, instead of going through the output resonator. It oscillated like the dickens and gave out 100 milliwatts. Well, the reflex klystron had been invented at that time, in principle, but this was a very simple tube. It had a 300-volt electron beam of about 20 milliamperes. It gave about 100 milliwatts or more out. And you could tune it by just tuning this one resonator.

Goldstein: You say it gave 100 milliwatts. What was the output of other klystrons?

Pierce: What I was working with at the time was the second harmonics from a triode oscillator, and I was lucky if I could get 10 milliwatts that way.

Goldstein: I see.

Pierce: This just appeared on my doorstep. Certainly, reflex klystrons had been invented and patented. I had never seen one. I think that they were making them

at Sperry.

Goldstein: I see. Did they also feature higher power output?

Pierce: Well, the others were high-voltage devices that looked as if they had been built by a German machinist, I once said, and that's true. They were pieces of laboratory equipment. And somehow I just stumbled onto this tube that worked marvelously.

Goldstein: Is the invention very simple?

Pierce: It's very simple; it's cheap to make out of simple parts. The resonator outside is very simple. And it became the leading oscillator used in all 10-centimeter American radars in the receiver for superheterodyne, or with frequency conversion. And it was called the McNally tube, after my boss.

Then the 3-centimeter radar was coming along, and we needed the 3-centimeter beating oscillator. And McNally wanted to build another like this. But I believed it to be impractical because the thing would just be too small, the resonator and all. But we did our best to build such a tube.

Meanwhile, a fellow had come to work for me, Gerry Shepherd--W.G. Shepherd. He later went to the University of Minnesota and became a vice president there in the general field of microwaves and electronics.

I thought that if you had a successful 3-centimeter oscillator, you'd have to have an internal resonator. And there was a lot of machinery hanging around, or a set of machinery, for making metal tubes, metal-based triodes and tetrodes and pentodes. There was sort of no reason why such a tube should have a metal envelope. The future of tubes still lay with glass envelopes largely, and this machinery was gathering dust.

I thought that one could design a tube with internal resonators to be built with this existing machinery. I asked Gerry Shepherd to do so as a task. Nobody told me really what to do, but I told him what to do. Many years later I found out

that because Gerry had a recent Ph.D., he thought that this was beneath him. He was a little offended at the time. But this was a job that was beyond the capabilities of the tube shop.

So Gerry Shepherd sat down and mastered the tube-making machinery, and somehow between us we cooked up a design for a 3-centimeter tube with an internal resonator. It worked just fine. We never made the glass envelope tube. This was called the Pierce-Shepherd tube because I was Shepherd's boss.

Later on we made a 6-centimeter version that had some difficulties. I can't remember its number. So the 3-centimeter radars then used the Pierce-Shepherd tube, the 723A.

Goldstein: I see. If you built the tube like this with an output of a few hundreds of milliwatts, what sort of range was that? What could you use that in?

Pierce: Well, it was used principally for the beating oscillator in the frequency conversion of the superheterodyne radar receiver.

Goldstein: Oh, I get it. So it would generate the frequency with which you would modulate a stronger signal.

Pierce: No, the weaker signal came in, and it was fed into a silicon crystal diode together with power from this 723A. You didn't really need the 100 milliwatts. That produced a difference frequency signal; the difference frequency was amplified, usually about 60 MHz as the intermediate frequency, by the vacuum tube amplifier.

Also the reflex klystron had electronic tuning. You could tune the frequency some 10 to 30 MHz by varying the voltage on the reflector without changing the resonator setting. And that was useful in automatic tracking the received frequency.

Goldstein: And was it really just an accident that you charged that plate negatively?

Pierce: That was an accident. Somebody had thought of reflex klystrons and had, I

guess, patented them. But my introduction to them was quite accidental.

Goldstein: Do you understand why, if they were patented elsewhere, they weren't being used?

Pierce: Not really. Probably because they were built by German machinists.

Goldstein: Weren't you telling me that in terms of the microwave work, you were following the progress of the tube through manufacture?

Pierce: Yes, in that case. The McNally tube was fairly straightforward, and it went to manufacturing. I don't know quite how. This metal 723A tube had been built on machinery that existed and was available for manufacture someplace else. It existed there. There were a number of problems as to how to change the resonator spacing--and how you tuned it. There was a guy named Vance, and I think it was his idea, that you had a couple of little metal strips, one with a left thread and a right thread, so as to push when you turned the screw. That would adjust the resonator. This was on one side of the tube. A fixed spacer was on the other side. I think that Vance, in the tube shop, was responsible for this differential drive mechanism. Or I might have been.

I had the crazy idea that if we secured this tuning mechanism to the outer envelope of the tube by soldering it on, with as small a distance from the top as possible, this would be less affected by the tube heating up or cooling. We built it this way because I was the guy who was deciding things. Then Vance got tired of that awkward thing and tried just fastening one end down to the base part of the tube, and the other upper part of the tube, and not trying to solder it on the outside of the envelope. And that was very much less thermally affected by turning it on and off than my crazy idea, which required soldering the thing in rather than just welding it in to the base of the tube.

Goldstein: I guess I want you to try to locate the source of the most important stage. Was the inspiration to do something completely different more important than

actually trying to implement the innovation? Did most of the improvement come in the development, the experimental, stage? Or was the concept more important?

Pierce: The concept was much more important. But its outcome was very strange because here I'd been struggling to get a few milliwatts of power at 10 centimeters from the tubes that were available to me. And here it just poured copiously forth from an accident. No, most of the things that have been an improvement I've done it's because it was a good idea in a broad way, and it succeeded. Not because it was so well done.

Goldstein: I see what you mean. All right. I see here that there's modifying the electron gun, and I know that eventually you came to develop what's called the Pierce gun. Was that here, or did this come later?

Pierce: That was part of the heritage of the unsuccessful work on deflection tubes. Electron beams in those days usually had currents considerably less than a milliampere; a few hundred--tens or hundreds--of microamperes and that in cathode ray tubes and other high voltage things. I was trying to get currents of several milliamperes at voltages of a couple hundred volts, or 300 volts. And that was difficult.

The story of the Pierce gun is this. It has to do with a space charge, which tends to push the electron beam apart and make it difficult to calculate anything. But there were solutions that others had found for electron flow in parallel lines between infinite parallel cathode and anode. And electron flow between the concentric spherical shells-- in or out.

Goldstein: And in three dimensions, right? It's radiating out in this dimension?

Pierce: This is now in three dimensions, yes. There was also two-dimensional flow, I believe. Well, I said, we've got a known electron flow, but it's no good because it's coming from every direction into the center. But couldn't we have this flow

in a conical form? The problem was that outside the flow the equations are LaPlace's equations without charge, and inside the flow the equation is Poisson's with charge. Anyway, in one place you have a space charge and a known flow in straight lines, associated with which is the known variation of voltage versus distance, or potential of distance. Outside of the electron flow you have the equations which govern electric fields in the absence of space charge. Can we have a solution of these chargeless equations that will match the solution of the equations with charge along a conical boundary?

Goldstein: Was the boundary an actual piece of metal there?

Pierce: No, that's just an abstract boundary. If there is no field normal to the boundary along the side of the cone you can have the same variation of field with distance as inside this space charge flow. This is a very complicated variation, so I had in those certainly pre-computer days, no way of computing a proper electrode. But one could take an moded the thing in an electrolytic tank with a sloping, non-conducting bottom. The water line with the non-conducting bottom is the axis of symmetry. An insulator at the angle with the axis represents the boundary of the electron beam. And the two electrodes outside the beam are shaped to give the right variation of voltage with distance along the boundary.

The motivation is to find the configuration for an electrode at the cathode potential, and for an electrode at the anode potential, such that if the potential along the edge is the same as the variation of potential from the concave--the spherical--cathode toward the spherical anode, such that this will produce a field here with no component normal to the boundary.

Goldstein: You see, I don't even know why you want that.

Pierce: So as not to deflect the electrons.

Goldstein: Ah!

Pierce: I want to get the same electron flow in a conical beam that one would get between the concentric internal sphere and external cathode.

Goldstein: Why do you prefer the conical beam as opposed to the concentric spheres?

Pierce: The flow between concentric spheres is no good because there's no way of the electrons getting out. So what I want to do is to have the electron flow just like between concentric spheres.

Goldstein: But only for a portion?

Pierce: But only for a portion.

Goldstein: Right. And this way you have more traditional cathode-anode interplay?

Pierce: Yes. You have a cathode and an anode, but you know what the relation between current and voltage is going to be. And you know that all the electrons will go in straight lines. Because you have a solution for this problem. Now you could try to get an analytical solution for the field of outside the beam, in the region where there isn't any space charge. One didn't have an ability to calculate this in the pre-computer days. So one knows that the right equations are obeyed in an electrolyte.

Goldstein: I think I understand the principle here. What you're saying is that you built an analog.

Pierce: An analog in the electrolytic tank.

I haven't thought about this for years. But anyway, here will be parts of the spherical electrodes. In this direction is a sector around the axis.

Goldstein: [Chuckling] I just want to make sure I understand. The point of all this is to defeat the space-charge effect of large currents coming off at the cathode.

Pierce: Yes. Especially near the cathode.

Goldstein: And that permits these higher currents?

Pierce: The worse thing about the higher currents, if you design things at random, things get started all wrong. This at least gets things started right, down at the

low-voltage end.

Goldstein: Well, had you done much trial-and-error tinkering before you'd developed this electrolytic system to test it?

Pierce: No, there was no way of trial-and-error tinkering without the electrolytic system.

Goldstein: That's an interesting solution. Did the analog occur to you?

Pierce: Yes, it occurred to me. I also had the help of a mathematician who got the right solution for infinite parallel planes. I think he pointed out to me this angle; I'm not sure. He was L.A. McColl.

But you could solve problems in those days by tinkering. This was very much like the analog for the electron flow in your rubber sheet thing.

Goldstein: Do you think you were unique in working with these analogs? Is that a common thing?

Pierce: I don't know. It was the only way of solving either of these problems in those pre-computer days. And it was a way that depended a lot on just getting a feel for it. In one case you wanted the ball bearings to go someplace. If you watched where they were going, you could see what changed. Now in the same way in trying to match the potential to a prescribed potential. You could sort of see what to change, if it wasn't right. Anyway, the Pierce gun came from work on the deflection tube, but it was just what was needed for guns for klystrons and traveling wave tubes.

Goldstein: Why?

Pierce: Because they demand well-formed electron beams so the electrons are all headed toward something.

Goldstein: Couldn't you also accomplish that with either electric or magnetic focusing?

Pierce: I don't think so.

Goldstein: No? I'm not making a suggestion. I'm just wondering why not.

Pierce: Once you get a beam of electrons that are going pretty fast and the electrons are all headed toward some point, then there's a theory of electron lenses and stuff that works. But when the electrons are down near a cathode with very low velocities, there's no hope.

Goldstein: I see. They boil off erratically?

Pierce: They go off heaven knows where. The problem was to get the electrons in a focused beam up to a fairly high velocity where they could be dealt with by conventional electron optics and electron lenses and electron focusing. Magnetic or electric.

Goldstein: I see. Was this widely recognized as a problem?

Pierce: Yes.

Goldstein: See, I like to explain the correct solutions to things in terms of somebody asking the right questions. So I'm wondering what questions you were asking yourself?

Pierce: Well, I was asking myself how to get a well-focused beam in deflection tubes; electron beams in general came later. And this was recognized as a good thing. There was an IRE meeting shortly after I did this and I gave a paper about this. But Browder J. Thompson, who was quite some person at RCA in those days, couldn't be there when the paper was being given. He asked me what it was all about, and I told him. And it was very much appreciated also out at Stanford where they were dealing with electron beams and didn't know how to get them started.

Goldstein: You patented this, and then Bell was at liberty to license it out?

Pierce: Anything that looked patentable in those days, you patented it--or Bell Laboratories patented it. The purpose was not so much to make scads of money as it was to have some patents to trade with other people at GE and RCA and so forth, so we could get the use of their patents.

Goldstein: So did that tend to homogenize all the innovations that came out of each of these labs because they were all sharing technologies?

Pierce: I don't know, but it could be very bad if you couldn't get sharing. For instance, the guy who invented the self-biasing of the triode amplifier by putting a resistor in the cathode circuit, made an awful lot of money out of this. Making money out of patents is sort of like making money out of the lottery. You really shouldn't expect to, but occasionally people do. But mostly patents are a means of protecting your position. That is, if you don't have some patents of your own to trade, you could be really held up.

Goldstein: How did that work? I don't know what department would swap these patents. Did they consult with you about which ones were important? Did they try to get you to judge which were the patentable ones?

Pierce: Anything you wrote up you automatically routed to the patent department. If you wrote a paper for publication, the patent department got a look at it. If they thought anything was patentable, they would insist on filing before it was released publicly.

Goldstein: Did they have their own technical staff to judge what was important?

Pierce: Yes. Well, they were pretty good technically--some of them.

Goldstein: I see.

Pierce: I remember the guy named Gunther who got the patent on this Pierce gun idea.

Goldstein: When you say he got it, you mean he handled it?

Pierce: Yes. They tried to get as broad a claim as possible. Poisson's is the equation with the charge in the beam. LaPlace's equation is without the charge, outside the beam. One of the claims was an electron gun in which Poisson's equation is used in the beam, and LaPlace's equation, outside. [Chuckling] Which is every possible gun. I don't know how he thought up those words. Sort of scrambling whatever was in the paper. I don't know how it ever got allowed. They can't

have been very bright in the Patent Office.

Goldstein: Did they work at changing the wording to almost fool the Patent Office? Or, if they wanted a broad patent, would they return to the inventor and say, "Hey, can you make it do this one other thing?"

Pierce: Oh, there's a certain amount of consulting with the inventor to try to broaden the claims. I would take credit for this if I thought it was due, if I thought I could get a claim like that. There's a certain amount of nonsense that goes on in the patents, as well as with all other things that have to do with the legal profession. [Chuckling]

Goldstein: Okay. Now, let's see, where are we now?

Pierce: Well, that was the Pierce gun.

Goldstein: Right. Was that this invention here in '38, or this paper in '38? Or are we working later on now? I mean, I imagine it probably corresponds to several of these papers.

Pierce: "Gun Design in the Presence of Space Charge." That's '39. Can that be it?

Goldstein: Did you just drop electron multiplication and deflection and start working on microwaves?

Pierce: Yes. It became popular. Let me go back to my published papers. "Rectilinear Electron Flow in Beams." That's 1940, though. Well, now, let's see. Here's "Limiting Current Densities in Electron Beams"At a given potential you can't just crowd more and more current through...." That was important to high-current beams also. "Rectilinear...," August 1940. That seems to be when I published the last. "Gun Design in the Presence of Space Charges." That was 6/14/39, and it was published in August1940, after being patented. But that is interesting to me because it was a real problem in deflection tubes which weren't any good for anything, but it was also a real problem in microwave tubes, which are good for something. [Chuckling]

Goldstein: Was that the connection then that led you from the deflection tubes over to microwaves?

Pierce: No. That was just some useful background when I got into microwave tubes.

Goldstein: Was there a critical event that made you say, "Enough with the deflection tubes. This isn't happening."?

Pierce: Well, there wasn't a critical event. It was just that things looked worse and worse, and it wasn't worth pursuing.

Goldstein: And there was room in Bell? Did you have to switch departments to get working in microwaves?

Pierce: Oh, no. I was told to work on vacuum tubes, so I went there, and that's what I did for years.

Goldstein: So you could wake up one morning and just say: I'm working on microwaves because I think that's got more promise?

Pierce: Yes, that's it. And I'm sure there was some microwave environment. Just like the coaxial cable environment that led to the false track of the multipliers and the deflection tubes. So there was interest in microwaves. At that time that was when the klystron was invented and papers were published on it and so on. I became aware of interest in the Holmdel laboratories in a microwave communication system.

Goldstein: Was there any feeling of disappointment regarding this other area, deflection tubes?

Pierce: I was certainly disappointed, and I was tired of it, too.

Goldstein: I'm just wondering if it was a melancholy choice, or was it charged with excitement? You know, because now microwaves seem so promising to the future.

Pierce: My recollection of the past, except on a few occasions, isn't that fine-grained.

Goldstein: I understand that.

Pierce: Well, then as far as my tube work at Bell Laboratories, I had worked on electron multipliers. I'd worked on deflection tubes. The electron multiplier was successful, but there was no internal market for it. The deflection tube was just not a good idea, but it produced the Pierce gun, which was a good idea. And produced a lot of thoughts about electron beams that were useful later on. Then I got into the swim of the microwave thing by trying to make microwave amplifiers. At the same time, Arthur Samuel, who died a few years ago, was around. He was an older-time employee than I. He had started to work on klystrons, high-voltage klystrons. As a matter of fact, they were used in a prototype TDX system as amplifiers, to be replaced by the Morton triode tube in the fifties. But he was working on microwaves.

I found out about microwave work at Holmdel. Something encouraged me--an mixture of internal inclination plus, perhaps, external exposure and happiness that if I did work on microwaves it would be good for something. I can't remember anyone telling me just to go work on microwaves. Harald Friis says in his recollections that at a certain point he was told not to work on short waves anymore, to get busy and work on microwaves, that he'd done enough on short waves. I don't remember anyone ever telling me this sort of thing. But there is more than one way of conveying a message.

Goldstein: Yes. [Chuckling] Do you remember anything subtle?

Pierce: No, I don't remember anything subtle at all. I remember finding out about the microwave work by visiting out at Holmdel and having people from Holmdel visit at my place.

Goldstein: You've mentioned a couple of times the freedom of the environment and the room that you had to work. Did your appreciation of that influence your performance as a manager? You know, you were saying that you told--I can't remember what's his name--there was someone whom you told--

Pierce: Shepherd.

Goldstein: Right, Shepherd. To work on the metal envelopes, and he thought that was beneath him.

Pierce: Well, he was being too much of a draftsman, a mechanic, you see.

Goldstein: I just wondered whether you felt obliged to give people the sort of freedom that you enjoyed.

Pierce: Well, I thought that people ought to have freedom. But I also never hesitated to expose people to what I thought were good ideas. I wrote something about freedom once. Essentially it says that freedom is wonderful, and you need freedom. But I guess that I remember the point in it was freedom is something to be made use of. It isn't just a cause all in itself.

Goldstein: Would you say that that was the attitude at Bell?

Pierce: Yes.

Goldstein: Or were you unique in feeling that way?

Pierce: No, I think that it was what I came to feel at Bell. I mean, people should be free to do things, but they shouldn't be irresponsible. Or, it's better if they aren't irresponsible. They'll accomplish more.

Goldstein: Sure.

Pierce: [Chuckling] I remember a story later on in Bell. A very bright guy came into my office (I was his boss's boss) and told me a wonderful idea he had had, and he was working on it. And I said, "Well, when will you be through with this? When do you expect to have it?" And he gave me a date which I wrote up in the corner of a blackboard, and I just left it there. And I always remember that he told me he would expect to have something at a certain time. But he forgot all about it. [Chuckling] Well, he was a very good guy, but I can't help feeling he'd have been even better if he didn't bite off more than he could chew.

Goldstein: You're working on microwaves. And does this lead naturally into the traveling

wave tube?

Pierce: Yes, it leads into the traveling wave tube. During the war microwaves were a big thing.

Goldstein: For radar?

Pierce: For radar. And we got all the reports of the Committee on Valve Development in England. The English are great on committees, and their committees are useful apparently. I could follow everything through reports. And we also got not only those reports, but many classified documents on information about microwave tubes. So I could follow whatever was happening in microwave tubes, really. And also we were in touch with the Radiation Laboratory and we saw Radiation Laboratory's reports. And I talked with others about klystrons and reflex klystrons and cavities, external or internal.

There was some calculation at the time, I believe--at least it occurred to me--that fields that ran along with the electron beam would be useful. And I calculated the acceleration of electrons at microwave frequencies, pushing ahead and back, for different electric fields through which they went. And the idea of a wave field that was traveling with the beam had occurred to me. But all I did was make calculations, and I missed the essential thing.

Then through a Committee of Valve Development reports, I heard about Rudy Kompfner's invention at the British laboratory of the traveling wave tube. He actually built something. That was where he excelled. The analysis he had was done for him by some theoretician, and it was very awkward. But he built this, and he found out that you got a lot of gain.

The important part of it was to take into account not only the effect of the traveling electromagnetic on the electrons, but simultaneously, to account of the effect of the electrons on the traveling field. You transfer energy to it. And unless you take both into account at the same time, you miss the point.

You take them into account, you get a growing wave.

Well, I had all the mathematical apparatus by then to treat this effect, and there were two things necessary to make the most out of the traveling wave tube, which was broad band and high gain and turned out to be generally swell. One of the things was this interaction taking into account at the same time, the interaction of the field on the electrons and of the electrons on the field.

What happens is that there are two electromagnetic waves, one going forward and one going backward, along a helix or other two-way transmission line. And there are two waves on the electron beam, one forward and one backward with respect to the electrons, but both going in the same direction. The slower wave on the electrons has a negative energy--this was discovered by a guy named L.N. Chu at MIT, and it's the easiest way of understanding what happens. It isn't how I understood it at first. I had just an analytical solution. So, you have four waves to begin with, before the electrons are coupled to the electromagnetic circuit. And you have four waves afterwards, which is a sort of conservation of waves. There's a backward electric wave on the circuit that practically doesn't interact with anything at all. Now let's see. There is a growing wave that is--an interaction between the slow, so to speak, backwards with respect to the electrons, electric plasma wave, and an attenuated wave, an interaction involving the circuit wave and the fast plasma wave, and an unattenuated mixed wave. The wave that increases exponentially with distance can do that because the electron beam has less and less energy as it goes along, and the electromagnetic wave has more and more energy. And that's what makes the traveling wave tube work.

So, you have a wave that grows in amplitude in both the electron beam and in the electromagnetic circuits. It's a composite electromagnetic, electric plasma, type of wave. But as the amplitude is larger and larger, the amount of

electromagnetic energy power is more and more, and the amount of kinetic power in the beam is less and less.

Goldstein: Right. Doesn't that slow the electrons down and then change the speed?

Pierce: On the average, the electrons are slowed down. That's where the energy comes from.

Goldstein: So the pitch of the helix has to change.

Pierce: Actually, it's advantageous to change the pitch of the helix. But the simple theory is a linearized approximation that I worked out. Other people did the nonlinear theory. You can go a long way by just a linear theory of the linear regime.

So there's a lot of gain in one. You start out with four waves, you must end up with four waves.

Well, anyway, the analysis of the traveling wave tube in terms of interactions between waves on the electron stream and waves on the circuits, which is the form it ultimately took was important. It was nice to have a theory. The theorist, Hatton, whom Kompfner went to produce a series analyses which gave very little insight into what was happening. The insight in terms of waves was very useful because it divided performance into two different parameters: how much of a loss or gain there was in going from the electromagnetic wave to the growing wave, and how fast the growing wave grew. And you could see that this was a better way of thinking about things.

Goldstein: Did it translate into concrete design improvements?

Pierce: It translated later into numbers, certainly. I don't think it varied with current and electron velocity and so on.

Goldstein: If you by chance stumbled into the optimal tube design, then would that analysis have been important? Or was the analysis important because it helped improve the design?

Pierce: The analysis was important because it told you more or less what you were going to get in terms of behavior with various parameters, physical parameters. And it told you what was going on. To get a good traveling wave tube, several things were needed. One was a good electron beam, well-focused. And the Pierce gun was very useful there.

Goldstein: How did Kompfner get it? Did he use the Pierce gun in his first model?

Pierce: I don't know. He probably got it by brute force.

Another thing was stability. Here you have a backward wave with no attenuation, practically, very little. And a forward wave with maybe 10, 20, 30 dB gain. Why doesn't it oscillate? The answer is that it does. Well, one of the first things I did was to calculate how much of the gain would be reduced by putting in loss--iron-plating the wires in the helix or something like that. Or putting loss material near the helix. And it turned out that for every of loss measured in a backward direction, a dB of loss put into the circuit itself, cost you only about a third of a dB in gain in the forward direction. So you could afford to put in a lot of loss, and you'd still get the forward gain. That was very important because it made the tube stable.

The analysis was very useful. Now the analysis had its limitations. As soon as you get there you ask, what will happen if you completely absorb the electromagnetic wave someplace along the line and let it build up from what's on the electron beam afterwards? I calculated that also. That leads to the idea of putting in a lot of loss someplace along the helix, smoothly enough so it won't reflect the waves, but short enough so that most of the tube is either in the input region or the output region. And that made things a lot better. Now you could put in just a lot of loss and make the thing stable. That made the traveling wave tube stable and good.

After that there were a lot of things to make it better. One of them was how to

focus the beam. We started out with a long solenoid. A coil, an electromagnetic focusing over the whole length of the tube. But this was burning up a lot of power for no reason.

The trouble with focusing with permanent magnets is that you need a uniform field over a long distance. But the magnetic field of a permanent magnet always spreads out to a distance radially that is comparable to the total length.

By this time Rudi Kompfner was in this country. How did that happen? I had seen him during a visit about a month long at the end of 1944--a visit to England for Bell Laboratories and also for the Navy. I went with a fellow named Homer Hagstrom. I met Rudi Kompfner, and that was essentially the highlight of the trip, although he didn't know it. After that, I about 1950--when was it? '50, '52, something like that--I had made the traveling wave tube work with a good deal of help from a young guy named Les Field, L.M. Field, who ended up as a vice president of Hughes as the head of their Malibu laboratory. When i first knew Les he worked on magnetrons, which was the glamorous thing at Bell Laboratories during the war. The traveling wave tube interested him more than magnetrons. In fact when the hostilities were over, the magnetron turned out to be good for nothing much but radar ranges.

So we both spoke at an IRE meeting, which was held at Yale and I don't remember the exact date of it, though it might have been around '50. And I saw that some guy who had been associated with Rudi Kompfner made a talk there. Because I thought this was a wonderful thing, and I think that Rudi deserved the credit.

Later on, sometime in the early fifties, Rudi was a senior civil servant who worked at Oxford, was offered a job at Bell Laboratories. He came back, and he had a very profitable association thereafter, working on traveling wave tubes and other things.

Well, I was talking about the focusing. That was a real problem. Rudi was interested in a lot of things, and at that time there was a lot of to-do about periodic focusing in cyclotrons. Rudi tried to apply this literally to focusing electron beams. It gave me an idea, a very simple idea. What if along the electron beam we kept changing the direction of the magnetic field? We'd still get focusing, but the magnetic field wouldn't spread out over all-get-out. The traveling wave tube is a good idea. But without periodic focusing of that sort it was limited. Such a periodic field can be produced in a number of ways. It was done first by Cal Quote. He used a stack of lot of little ferrite rings with a hole, and magnetic washers between them. The field was minus-plus or north-south, if you wish. North, south, north, south. And this again is like night and day in the business of focusing the beam. With very little magnetic material, you can focus things very nicely.

Goldstein: I see.

Pierce: Traveling wave tubes have been a great thing ever since. They're still used. There are a few remaining tubes that are still used in some of these communication satellites.

Goldstein: Do you remember why it was that you never got around to trying to build one of these traveling wave tubes when the idea first occurred to you before Kompfner's success? Were you busy with something else?

Pierce: I know the reason I didn't build something is that I didn't see anything attractive to build. I had made analyses of waves acting on the electrons and the electron beam, but I hadn't made an analysis to include both the action of the electromagnetic field on the beam and of the beam on the electromagnetic field. The important thing happens when you take both those things into account. That's what leads to a growing wave. And I hadn't thought of a way of getting a growing wave.

Goldstein: So once you saw the phenomenon, you understood why it was happening. But you just hadn't anticipated it.

Pierce: That's right. I just missed the boat. A lot of people missed the boat even more. They didn't know a good thing when they saw it.

Goldstein: [Chuckling] Is that true? Did people instantly understand the potential of the traveling wave tube?

Pierce: Well, when we explained it to them at this IRE conference.

Goldstein: Which you said was about 1950, since it was after the war.

Pierce: Yes.

Goldstein: You were working on microwaves as early as, say, '38 and '39. I've always had this impression that microwave research awaited the cavity magnetron. That you couldn't get anywhere until they had high power.

Pierce: Well, it's the radar that awaited the cavity magnetron, not microwave research.

Goldstein: I see. Was the cavity magnetron a big shot in the arm for all areas of microwave research?

Pierce: For radar. It was not much good for anything else. The cavity magnetron very efficiently produced huge pulses of microwaves. They weren't phase-coherent. The future of radar lay with phase-coherent pulses you could produce by klystrons and such. The efficiency of the magnetron was very important during the war. You got a lot of bang for a buck and per cubic centimeter and everything else.

After the war, there was a lot of effort to make magnetron amplifiers. But electron beams and crossed electric and magnetic fields need no encouragement to do something. They're fundamentally and inherently unstable. And all the efforts to make magnetron amplifiers, the carcinotron that CSF made in postwar years, were all sort of marginal in performance. They tended to produce noisy output. They produced dirty signals.

Goldstein: Were you at Bell Labs in a position to discourage research in that area?

Pierce: It needed no discouragement. [Chuckling] The people struggled with it a little bit and gave up.

Goldstein: You said--or I'd asked (I don't want to put words in your mouth) if the microwave research led directly to the traveling wave tube. But it actually sounds as though it was Kompfner's invention.

Pierce: It was Kompfner's invention.

Kompfner left Austria because he was Jewish, at the Anschluss or before it; I'm not sure which. He was trained as an architect, and he worked for an architectural firm. But his heart was in electronics and vacuum tubes, and he studied on his own. His studies got a great boost when he was interned during the early part of World War II. He studied with German physicists. And then, through people who knew him and his wife, he got un-interned and was sent to where the most highly classified radar work, the magnetron work, was done. He was sent to that laboratory. But there he didn't work on magnetrons. He went and invented the traveling wave tube and seemed to be going against the stream somehow.

Goldstein: But then once you found out about that, you said that you had access to that information. Did you drop what you were doing and start working on traveling wave tubes?

Pierce: Yes. [Chuckling]

Goldstein: And that occupied you up through the end of the war?

Pierce: For many years. Many years.

Goldstein: In your autobiographical sketch you start off with ECHO. Most of the attention that you received in those days concentrated on the communications satellites. But I wonder how you judge your work on tubes.

Pierce: What I did in satellite communications was superficial and short-lived, except

that in my position and with my enthusiasm, by a miracle I managed to get ECHO launched. But my technological contributions to satellite communication are very small compared with what I did about tubes.

Goldstein: So you did more of a galvanizing effect ?

Pierce: Yes.

Goldstein: Let's talk about that. How did your position your influence within Bell Labs improve? What's the story behind your climb up the ladder there?

Pierce: The vice president in charge of research I liked very much, Ralph Bown. He was the person who taught me about Holmdel and other things. So I knew him. It was wonderful how one knew people over various levels.

In Bown's organization of research, in vacuum tube research, I was put in with a guy named Sid Millman as a joint director of the vacuum tube research, I think because he distrusted me as a wild man. [Chuckling] I don't have a real reading on this. But when Bill Baker became vice president in charge of research, I was rapidly notched up a couple of levels. He once told me that Ralph Bown had blinders on. But I had got around--seen a lot of other things--in microwaves.

I was in charge of this small vacuum tube research area, along with Sid Millman. But I knew people in other areas. This is my conclusion. I had made a rapid transition of a couple of levels to a general department head, which is next to an executive director--and they put in my division anything that needed encouragement. And that included a lot of different things at various times. These things came and went. But all in all, it included vacuum tubes, microwave research, guided wave research--that was waveguides, which never came to anything. Well, there was switching research for a few years. There was computer research. There was speech and hearing. There was behavioral science, and economics. And I would learn a little about these things and be

helpful, I hope, to the people who were in my areas.

Goldstein: When you say economics, did you ever consider the economic viability of a particular product or innovation? Was that your responsibility?

Pierce: No, that wasn't. The economics research at that time in Bell Laboratories was the sort of thing that bore on regulation and pricing in a general way. It was made up partly of imports (I don't remember the names of those), and partly of reconversion. Ed Zajac was an applied mathematician. He became quite an economist and wrote a book on some aspect of regulation.

Goldstein: I'm not really talking about the activities of the economics department. But would you stop working on something if you began to think that--if it had some scientific interest but you didn't think Bell could use it as a product?

Pierce: That happened. I don't know that I ever stopped anyone working on anything. But sometimes people disappeared. Let me tell you one story.

This has to do with psychology rather than economics. They decided to bring psychologists into the Bell Laboratories research area. Thought it might have something to do with the Bell System. Research at Bell Laboratories always had something remotely to do with the Bell System, although sometimes it was very remote. Penzias and Wilson and the thermal radiation in space was pretty remote. They had physics and chemistry and physical chemistry and general areas of materials research. But in these areas there were things that were conspicuous--respectable physics and so forth and chemistry some aspects of science were conspicuous by their absence because they had nothing to offer the Bell System.

They got psychologists of various kinds. It was for some years in charge under me or indirectly under me and Ollie Holt, who I was under me. He was a sort of applied psychologist. He'd worked for the armed forces, and he was an interesting good guy.

Then another guy they got was Roger Shepard, who, if he didn't invent multidimensional scaling, practically did and helped popularize it. He invented the Shepard tones that go up and up. And he's now a professor of psychology here at Stanford and participates in this course I mentioned in which I give four lectures a year. He gives four a year. There was also an animal psychologist who worked with pigeons. And there were a couple of social psychologists whose names I can't remember.

There are two things about the psychologists. One is that this seemed odd to people at AT&T, and they had an investigation for three days, because this was something that Bell Laboratories didn't do, presided over by Fred Kappel, who was later chairman of the board of AT&T; he's the only board chairman that I really got to know--but at that time he was president of the Western Electric. And so we paraded our wares for two or three days, and we were found acceptable to him.

But how did it work out? Some of the psychologists just fitted in every. Roger Shepard did; he later left under his own free will. And generally the experimental psychologists, as the cognitive psychologists were called in those days, found something to do and interacted with the people in speech and hearing and with other people.

There was a fairly well-known social psychologist who didn't seem to interact with anyone. There were two social psychologists. Neither did the other one. The animal psychologists just sort of vanished after a while. It was interesting, but there wasn't any mutual fit of any kind.

So I managed to arrange that the well-known social psychologist should go out to the southern division of the Bell System in California, which was presided over by a vice president that I knew and be privy to all their conferences and discussions that he chose for--I think it was--several months. And he did this.

I didn't know what I expected he would do when he came back. Perhaps I thought that he would go and be just hot to do something that would save the Bell System or improve it. But his idea was sort of offhand. If we hired one or a few social psychologists, not as prestigious as him of course, why, he could see that there were several ways in which they might be useful to the Bell System. Of course he himself was above being useful to the Bell System. And he went back into academia and wrote a book, I believe, about abolishing war or something important like that. In the Bell Laboratories things didn't last long if they didn't seem to have some relevance to something.

Goldstein: At what level was the justification made? Did an individual have to?

Pierce: Sometimes I went along with things longer than my boss would.

Goldstein: So you would pitch it? You would continue to argue in favor of it?

Pierce: No, I just wouldn't argue against it. [Chuckling]

Goldstein: I see what you mean. All right.

Pierce: I think I had a lot of influence in Bell Laboratories, but it was as a person more or less trusted by the management that I had my influence rather than directly from my position.

Goldstein: It was a function of your personal relations with your higher-ups?

Pierce: Yes.

Goldstein: Is it because you'd proven yourself to be responsible?

Pierce: I was more responsible than the management had earlier felt that I was.

Goldstein: Right. And also you had a track record of good judgment.

Pierce: I had a track record, yes.

Goldstein: Do you want to just pause for a second here?

Pierce: About six months ago I had an idea concerning government-supported research. I won't peddle it to you unless you insist on knowing. [Chuckling] And I wrote several people about this and got a charming letter from W.O. Baker who

retired as president of the Bell Laboratories quite a number of years ago. Bill is a person whose letters are not always clear to me, but there's always clearly something on his mind. If you want to read a couple of paragraphs of it.

Goldstein: All right. I do want to look this over. I guess I can stop the tape. [pause] You just showed me a letter showing me that's an approving assessment of your capacity as an assessor of importance and viability for technology. This leads me to question the executive leadership at Bell. Did they generally speaking come from the low ranks? Had they served time as engineers?

Pierce: Generally, right up to the top of AT&T, let alone the Bell Laboratories, the management was technological and good. If I think of all the management people in the research department, they were all good technical people. Some of them very notably good.

Goldstein: When you say "good," they'd done important--

Pierce: Important work in research.

Goldstein: Engineering work?

Pierce: Yes.

Goldstein: That's interesting, because it's just a difficult choice to make. These people are productive as engineers, and you might think you want to keep them as engineers. What was the mechanism by which they bubbled up to management roles?

Pierce: Deep need. People who are both good technically and have good judgement. It was very important to Bell Laboratories in the development departments, which are about nine tenths of it, as well as the research departments, to have good technical leadership. I think that there is nothing that high technology can succeed at without good technical leadership at all levels. The problem was never whom of these many candidates shall we choose? But always, who can do the job? And I think of some very good people who didn't do all that well.

Goldstein: You mean as managers?

Pierce: As managers. I think I did very well at the level at which I was. I don't think it would have been any favor to me to have made me a vice president.

Goldstein: That's interesting. Why do you think? The job expanded in complexity?

Pierce: There's more crud that goes with the job the higher it is.

Goldstein: Sort of like in academics where you get these administrative responsibilities?

Pierce: Yes. I had what seemed to be administrative responsibilities when I was executive director. There was a process of, oh, going over budgets and giving raises. Bill Baker at that time was the vice president in charge of research--me and the other executive directors. But a lot of the stuff could be pushed off by me on people lower down the line. And I think the higher you go, the less easy it is to effectively push things off.

Goldstein: Consolidate the authority.

Pierce: Yes. I don't know. I didn't make a deep study of these things. I just lived it. About Bell Laboratories, I can say some things without any question. One is that it's very difficult to find people who are both good technically and are good managers and tend to get along with their people rather than quarrel with them, or tend to take the right responsibility. It's very rare, and it's hard to fill high-level jobs.

Goldstein: Do you think the quality with which that difficult job had been done was uniform over your stay at Bell Labs? I'm asking, was there a golden era of management expertise?

Pierce: I can best speak to that in terms of the people. Mervin Kelly was a very active manager. He became president not too long after I came to Bell Laboratories; I don't remember just when. He did many fine things. And one is in connection with the transistor. He fostered the work on the transistor. And after it was invented, things were either new art or they weren't. Vacuum tubes weren't new

art. New art was anything with transistors. People got very little credit for doing things that didn't have transistors in them. It's hard to move a massive-- How many people were there in the Bell Laboratories at that time? Seventeen thousand and about 1500 in the research department, something like that. Very hard to move the culture. But he did everything he could to move it.

Goldstein: He didn't want to hear from old art?

Pierce: Yes, he didn't want to. You just weren't supposed to get any credit for old art. He almost did satellite communication in, but he retired just in time. He may have been right about that.

Jim Fisk, who followed him, was a real gentleman. He was technologically good. He was a nice guy. He wasn't as forceful. Julius Molnar was. Molnar was the executive vice president and probably would have been president of Bell Laboratories if he hadn't died of cancer. Molnar was the father of the picturephones. That wasn't an example of good judgment. Jim Fisk was followed by Bill Baker. And Bill Baker was followed by--gosh! his name is in this thing. I don't know it offhand. I'm sure I can find it. Bill was very active in pushing various areas and various people. You wouldn't want him down on you. And he spoke in a sort of fashion that wasn't always easy to understand. But you got a message if there was a message there for you. He avoided any messages that would turn things abruptly on or off. Particular things. He's a great guy. I think that he had a more active sense of management of Bell Laboratories than Jim Fisk did. It couldn't have been more active than Kelly's. [Chuckling]

Goldstein: Oh. Oh, yes. Thank you for using Kelly's name. That reminds me, I was very curious to know how this new art/old art idea of Kelly's affected your role as a vacuum tube man.

Pierce: Well, I was getting out of vacuum tubes, and I wasn't in the forefront of Kelly's

glasses, I guess. Bill Baker was more of a champion of me than Kelly. The new versus old art was sort of over by then.

Goldstein: I see. But in the late forties and early fifties, was there still research going on in the basic triode and pentode?

Pierce: No, there wasn't. And let me tell you the horrible thing that happened in the development department. Most of human progress is improving on what you already have. And it's done by some sort of magic. The people who improve things take small steps, and the things get improved.

When I was working on millimeter-wave klystrons shortly after the war, I missed out on the 1- 1/4-centimeter klystron. Vic Neher did it right, and that was the one that was adopted. He was at the Radiation Laboratory.

So I tried to make a 6-millimeter klystron out of the approach that I had used for the 1-centimeter klystron--out of the leftovers. I did, and it worked. I made a few that were used at Holmdel for making 6-millimeter rain attenuation measurements and various other things. Then I wasn't in the business of making them anymore, and the Holmdel people tried to get some made by the development department.

It was turned over to a person who was an expert in making high-performance pentodes and he sort of threw something together without any instinct. For instance, I had found out that with copper-plated steel you might as well leave the plating off for all the good it does in improving conductivity. If you wanted to get the conductivity of copper, you have to use copper. There may be some way of plating things that will do, but not just ordering them plated. And they tried to make some tubes, and they just didn't work.

Well, if they had been improving working millimeter wave tubes, they would have improved them. They were just doing things not knowing what they were doing. That was bad and it doomed to failure.

There was a nice guy in the development department who was an analytical thinker. I don't remember his name offhand. He looked at the problem and made much better millimeter wave klystrons than I'd made by not making egregious errors. But there's the trouble with improving things; that if you get in unfamiliar territory, you sink.

Goldstein: I agree with what you're saying, but I'm thinking of some of the examples that we've talked about already, and in some places there are differences. For instance, in the reflex klystron, that wasn't an improvement.

Pierce: That was a major upset, yes. That wasn't an improvement. That was a turning point.

Goldstein: And then there's future improvement.

Pierce: It's good to get hold of good things early because things that are so good, they'll survive minor mistakes.

Goldstein: Right. You're saying if it's conceptually sound, you can't take too long.

Pierce: We were trying to find out when I wandered off from vacuum tubes.

Goldstein: Yes. Once the transistor exists, was there was the feeling that tubes were moribund?

Pierce: Yes.

Goldstein: People just dropped work. Well, it sounds like you're saying that there was no fundamental improvement work left to be done on pentodes.

Pierce: That's about the size of it. The Morton triode was the last improvement in triodes.

Goldstein: Okay.

Pierce: Here's something in '49, the end of '49: "Composing Music by a Stochastic Process." Well, "The solenoid delay-line short-wave microwave radio...." By the end of 1949 I was pretty much out of tubes. I wrote "Microwave Tube Prospects" in the middle of '50.

Goldstein: And I have certain review articles that you wrote in the early fifties that deal with tubes.

Pierce: Yes. I had gone into microwaves and other matters by that time. Let me tell you two real flops: One was the double-stream amplifier, which I thought was an absolutely marvelous invention. I told you how the traveling wave tube works with one electron beam and one circuit, one electromagnetic circuit. The double-stream amplifier worked with two intermingled electron beams of different velocities and gave gain. This seemed to be just absolutely wonderful. It got rid of the circuit.

Well, there was a minor business in getting two intermingled electron beams, but the real trouble was you hadn't gotten rid of the circuit. You had to have a circuit to put a signal on, and you had to have a circuit to take the signal off. And all of these were complicated by having two electron beams instead of one. So it was absolutely a blank as far as any good use goes.

Goldstein: It occurs to me here's something that I'd really like to know. Okay, you say that sounded like a great idea. Was your enthusiasm for it based on the fact that it eliminated a circuit?

Pierce: Yes.

Goldstein: Is that the sort of economy that you like to see?

Pierce: Oh, that's right. It seemed just wonderful not to have to have a circuit.

Goldstein: If I look at the reflex klystron as compared to a normal klystron, it's got just the one resonator, so there's another economy. Is that something you'd say that you appreciate in components?

Pierce: When I invented it I was trying to deal with the thermal velocity distribution of vacuum tubes--microwave tubes--and what its effects would be. So not being able to handle the problem, I cut the number of streams down to two and found growing waves. I thought, Oh, a wonderful amplifier. And it wasn't a wonderful

amplifier.

Goldstein: Because?

Pierce: Because there was nothing good about it in particular. You really didn't get rid of the circuits. You still had to have them at the beginning and the end. It was complicated by having to have two separate streams of electrons at different velocities. There just wasn't any real advantage and lots of disadvantages.

Goldstein: But the thing that got you working on it in the first place was the potential of eliminating the circuit?

Pierce: Well, the thing that got me working on it was the idea of gain without circuits, with just electron streams, that I'd stumbled on trying to work out thermal velocities. You could utter a lot of words that would make this seem good, but it wasn't. The other thing that I noticed was Clogston cables. In '50, 4/12/50 I wrote a memorandum, "A Simple Explanation of the Lack of Skin Effect in Clogston's Mediums." This was in the days of coaxial cables. And coaxial cables were lossy because of the skin effect. Maxwell's equations tell you that the current won't penetrate very far into the surface of the conductor at high frequencies. Clogston found a way of getting all of the conductors to share in the current carrying. And it consisted of a lot of very thin conducting films separated by layers of insulation. It's a medium, a particular medium.

Goldstein: Why don't you get capacitance between these successive layers?

Pierce: Well, if you work it out through Maxwell's equations, you find out that under very special conditions these all sort of share equally in conducting the electricity.

Goldstein: Okay.

Pierce: Now a lot of research was done on this. It was regarded as very promising because coaxial cables were very valuable. But it was hard to realize it, hard to build it, hard to make it work. You wanted one mode in this medium with a

transverse distribution. But there were really lots of modes in it, many of them with high attenuation. And it was very ticklish. You imagine a hundred layers or so, there'd be a hundred different modes in that case. The other thing is, when all is said and done, it didn't buy you all that much. As I remember it, you have an attenuation that increases with frequency. What you manage to do is flatten out the attenuation over some frequency range rather than having it low at the low-frequency end and high at the high-frequency end. It sounded like just an absolutely wonderful idea, and a lot of people worked on it. In any practical sense, it wasn't a practical idea. And in the long run it really didn't matter because optical fibers came along. So it's one of those things that looks so attractive--to some people anyway--as did the double-stream amplifier. And I admired it, and various people worked on it, and nothing came of it.

Goldstein: I understand what you mean when you say it looks attractive, and I wonder if we can say something generally about your values with regard to these things that made something attractive or not attractive.

Pierce: If it works just a lot better without too much trouble. Both the reflex klystron, as opposed to trying to make triodes oscillate a function up in those regions, and the traveling wave tube, as opposed to other microwave amplifiers, were that sort of thing. They worked better than any other that was available then.

Goldstein: Now if it works better and costs more to make, do you think it's worth paying extra prices for your equipment if you get the superior performance? Generally, I mean.

Pierce: All I can say is--weasel out--by saying that's a matter of economics.

Goldstein: It's true. But I just wondered if you were impatient about the economic concerns.

Pierce: There's a lot of talk about including manufacturing processes. And if you can make them different in kind, as I think automation to some degree has made

them, this is a really big breakthrough. There comes a long period in either technology or processes for doing things in which you're getting things a little better and a little better and a little better and a little better. That's awfully important. It's how you live in the face of competition. But there are other things where you just plain change something, and that can be true in the product itself. The reflex klystron is better for its purposes than the triode--and cheaper. It's easier to do. This could be true in manufacturing technologies, too, I suppose. Well, it was Henry Ford and the assembly line. That was a major change that gave him a real advantage against other competitors. I spoke about the improvement of triodes in the Bell Laboratories development department, or the improvement of tetrodes and ordinary vacuum tubes. It was important to improve them. But having a transistor or something is still better. Most, I think, of engineering and technology goes into improving things once you have them. If we waited for major breakthroughs, so to speak, to improve and make the world better, there aren't enough per year or decade.

Goldstein: Yes, I wonder if that's true, because then I'd agree wholeheartedly. If it's true because so much advantage can be had through minor step-wise refinements, or if it's because those refinements suggest the next breakthrough. Do you think--?